The question of what degree of evidence a single bloody smudge may give of the identity of some supposed miscreant with a convict having a previous official record is a matter for dispute. Still more is that of a case where, say, the right forefinger of Richard Roe may be practically identical with the left ring-finger of John Doe. Such similarity would be no evidence whatever for personal identification. In labouring to be brief I trust I have not been quite obscure. Sir William, in his review of the history of this discovery, has not made any reference to my little contributions on the subject. He, however, did acknowledge my priority of publication in your columns of November 22, 1894, and for that "gift granted" I must feel grateful.

HENRY FAULDS.

Stoke-on-Trent.

I HAVE to thank you for your courtesy in forwarding me a copy of Mr. Faulds's letter to you, and, in compliance with your request, I submit the following remarks.

The only point I feel bound to notice is his complaint that I have not mentioned his name in my story of "The Origin of Finger-Printing." Mr. Faulds's own account of his claim has been so fully placed before the public in his letters to you from Japan, of October, 1880, and later, that I think I was right in keeping to that period of history, twenty years further back than his, which lay within my own knowledge.

But his present letter breaks through all bounds of social courtesy, and it is only his position as a professed man of science that justifies me in correcting him. Mr. Faulds has the temerity to scout my statement that I was moved to study finger-printing by the fascination of Kōnāi's hand-mark (taken as it was for the same purpose as finger-prints now are). The finger-tips were badly smudged, but the small furrows on the palm were exquisite, and moved me to take better impressions than his from my own fingers, as I tell the reader on the same page, only Mr. Faulds ignores it. This is not the spirit of science.

I will now, with your permission, show reason why I could not honestly have introduced Mr. Faulds's name. His letter of 1880 announced that in the previous year his attention was directed to the peculiarities of finger impressions on pottery, and that he had come to the conclusion, by original and patient experiment, that finger-prints were sufficiently personal in pattern to supply a long-wanted method of scientific identification, which should enable us to fix his crime upon any offender who left finger-marks behind him, and equally well to disprove the suspected identity of an innocent person. (For all which I gave him, and I still do so, the credit due for a conception so different from mine.) But he went on to say:—"There can be no doubt of the advantage of having a copy of the for-ever-unchangeable finger-furrows of important criminals."

This expression made me protest at once, in my reply, that I could not understand how, in less than two years, he could have come to the knowledge that the furrows were unchangeable. It had taken me nearly twenty years of sustained experiment to demonstrate this persistence of the patterns for at least fifteen years of a man's life, and it is plainly impossible for any man with a scientific turn of mind to put this doctrine forward after only twenty months or so of experiment. My reply, therefore, of 1880 expressly challenged his authority for the statement, and he has never justified it. My challenge did oblige him to meet it as best he could, but the nearest approach I have seen to an answer is the following extract from an article of his in *Knowledge*, April, 1911:—

"The mode I took to test whether the ridges ever shifted their situation or changed their form was by shaving away their elevations . . . having first taken careful imprints of the patterns. After the skin grew up again, fresh imprints were taken and compared with the old ones, . . . but in many hundreds of cases, tested thus three or four times, not one solitary example of a variation in pattern was detected." His return to England broke the further investigation. He goes on to say:—"The firm conviction, however, was established in my mind, which nothing has occurred to change, that skin furrows for the purposes of identification are invariable throughout life."

This quotation is his latest statement of his authority, but it needs to be read with an extract from a previous letter of his, dated June 5, 1909, in which he says:—

"One of my earliest experiments was to shave off the ridges of the finger-tips with razors; the pattern on the skin was reproduced with quite unvarying fidelity, unless part of the true (deep) skin was removed."

I take it that this is the only foundation he has for his claim to have known the law of persistency in 1880. I leave it to men of science to judge whether his experiments sufficed to prove persistency of a finger pattern for life.

W. J. HERSCHEL.

Warfield.

The Date of the Introduction of the Term "Metabolic."

THE concept and the term "metabolism" have played such a prominent part in the development of physiological science that it should be interesting to know by whom, and when, the term was first used. Prof. Bayliss, in his "Principles of General Physiology" (1915, p. 263), says that, so far as he can discover, "metabolism" was first used by Sir Michael Foster in his "Text-book of Physiology," the first edition of which was published in 1883. It seems, however, that there is a still earlier use of the term in the writings of no less well known an investigator than Theodore Schwann, enunciator of the cell-theory. The passage I allude to occurs in the chapter called "Theory of Cells," the last in Section III. of Schwann's classic, "Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants, by Dr. Th. Schwann, Professor in the University of Louvain," published in Berlin in 1839. My translation of it is that made in 1847 by Dr. Henry Smith, of London, for the Sydenham Society; it runs thus (p. 193):—"The question, then, as to the fundamental power of organised bodies resolves itself into that of the fundamental powers of the individual cells. . . . These phenomena may be arranged in two natural groups: first, those which relate to the combination of molecules to form a cell; secondly, those which result from chemical changes either in the component particles of the cell itself or in the surrounding cytoblastema, and may be called *metabolic* phenomena (τὸ μεταβολικὸν, implying that which is liable to occasion or suffer change)." The italics are in the original. Here, then, so far as I know, is the first use of the term "metabolic," though undoubtedly not the first occurrence of the conception of chemical changes in living matter. Schwann uses the term "metabolic" exactly in its present-day sense, the phenomena of change, interchanges, of material in and by living matter.

The year 1839 may be taken as the date of the introduction into biological terminology of the expression "metabolic," and the person Theodore Schwann, at one time professor in the ancient University of Louvain.

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As soon as I came across Prof. Bayliss's statement I wrote to him pointing out what seemed a use of "metabolic" earlier than 1883. Not possessing the work in the original, I asked Prof. Bayliss what was the exact word translated "metabolic," as I wished to know whether it was any derivative of "Stoffwechsel," the present-day German word for "metabolism." Prof. Bayliss replied at once to say that he had found the original passage, which he kindly transcribed for me as follows:—"Zweitens, Erscheinungen, die sich auf chemische Veränderungen, sowohl der Bestandtheile der Zelle selbst, als des umgebenden Cytoblastems, beziehen, diese kann man *metabolische* Erscheinungen nennen (τὸ μεταβολικὸν) was Umwandlung hervorzubringen oder zu erleiden geneigt ist."

"Metabolische," and not any derivative of "Stoffwechsel," is, therefore, the word employed. This is not the only passage in which Schwann uses the word; on p. 197 he speaks of "metabolic power," and again later, in contrasting a crystal and an organism, he remarks that the metabolic properties are "quite peculiar to cells." The word occurs twice more at the close of this remarkable chapter.

As the origin of anything cannot fail to be interesting, and as the word "metabolism" is so very much used in biological exposition, I have thought it well to ask you to publish these remarks.

D. FRASER HARRIS.

Cultural Amœbæ from the Intestine of Man.

I SHOULD like to thank your reviewer for his commendatory remarks in NATURE of December 21, 1916, on the account of the parasitic Protozoa which I contributed to the "Animal Parasites of Man." Without in any way wishing to raise a discussion, I think it should be pointed out, in respect to his statement about *Amoeba limax,* that much information concerning these cultural organisms that may occur in the human intestine will be found under the designation "cultural amœbæ" on pp. 42, 618, 742 and 743 of my work. As your reviewer rightly infers, *A. limax* is not now strictly the name of a single species, but rather of a group or type of free-living forms which show differences among themselves, but can be cultivated on artificial media. It is not easy to point out marked differential characters between them and Entamœbæ, especially after consideration of the work of Drs. Williams and Calkins, to which reference is made by me on p. 42 and pp. 742 and 743 of the book.

H. B. FANTHAM.

Cambridge.

DR. FANTHAM has given information concerning the culture of "cultural amœbæ," and mentions (p. 618) that they are non-pathogenic, but beyond the statement (previously overlooked), on pp. 42, 743, that they exhibit morphological variations there is no note of their characters. The account does not therefore afford adequate help to those who, during microscopic examination of a stool, find for the first time amœbæ with a very large karyosome, and desire to know what they are. Although amœbæ of the *limax* type exhibit variations under different methods of culture, as described by Williams and Calkins, reference might have been made to the striking karyosome by which amœbæ of this type, as found in the human intestine, are often distinguishable. In view of the frequent references in recent literature to "amœbæ of the *limax* type," some account under this designation might have been given, *e.g.* on p. 42, following the account of the morphology of species of Entamœba from the intestine.

THE REVIEWER.

FLOUR STANDARDS.

REFERENCE was made in NATURE of November 23 and 30, 1916, to the Order directing that millers shall increase the yield of flour from wheat by about 5 per cent. The result was that the corresponding proportion of "offal" was mixed in with the flour. An Order has now been made to the effect that a further 5 per cent. *must* be added to the flour. This may be done either by taking another five parts of offal for that purpose, or (and this is a most important new departure), at the miller's option, by adding five parts of flour derived from barley, maize, rice, or oats. In addition, he *may* add voluntarily another five parts, making ten parts in all over and above the previous increase in flour yield of seventy-one to about seventy-six parts of flour from 100 parts of wheat. The result is a compulsory 10 per cent. or a voluntary 15 per cent. more bread with the use of the same quantity of wheat.

In his article in NATURE of November 30, the present writer deprecated any further addition beyond the first 5 per cent., except in case of dire necessity, because of the deterioration in quality of the resultant flour. The necessity has apparently arisen, but the use of flour from other cereals is a valuable alternative in the present difficulty. Except for the absence of gluten, barley and the other flours indicated will not differ greatly from wheaten flour. They will not of themselves have rising power, and consequently the gluten of the wheaten flour present will have to buoy up the whole loaf during fermentation. But, on the other hand, there will be an absence of the proteolytic and starch-converting enzymes which are so active in the whiter portion of the offal of wheat, and act so adversely on the gluten and starch of the flour.

So early as January, 1915, the *Lancet,* in a leading article, suggested the use of cornflour in order to eke out the wheat supply. In consequence some experiments were made by Lieut. W. Claude Jago, the results of which were published in the *Lancet* of February 13 of the same year. Loaves of bread were baked from (1) London household flour only, (2) a mixture of ninety parts of the same flour and ten parts of cornflour, and (3) a mixture of eighty parts of the flour and twenty parts of cornflour. The bread from the mixtures was slightly less in volume, but fairly equal to that from the flour only in texture and appearance. This judgment was confirmed by the editor of the *Lancet,* who stated that the loaves "appeared to be quite acceptable." Cornflour is, of course, maize starch, and is manufactured very largely in the United States. If this variant of maize flour be permitted, its employment will result in a considerably larger yield of bread and of a type which will accord with the reasonable tastes and requirements of the British public.

WILLIAM JAGO.

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